view. That Gray's 'Manual' is often used for the mere determination of names of plants does not interfere with this its higher and primary use. This distinction 'A teacher' seems to ignore. If he will call to mind that it is not from finding out mere names of objects, or giving them, but from weighing and discussing the nature, meaning, and causes of the relative affinities of organized beings, that the whole philosophy of natural history has arisen, he will perhaps agree that it is not best to teach pupils to think that they have gained the least knowlege of nature, when they merely know what their elders name a given object. The name may be called a necessary evil; and unless, with it, is more emphatically acquired a knowledge of the structural and biological relations of the object which it bears to other objects, it is worse than useless knowledge. This idea should underlie every manual for instruction.

SAMUEL H. SCUDDER.

Coloring geological maps.

Having occasion recently to have printed a miniature geological map of Indiana, I endeavored to use the colors recommended by the International congress of geologists. Supposing that my endeavor might be more or less suggestive to those interested in the subject, I sent specimens of the map to the members present at the Berlin meeting of the congress, and with them a letter in which I pointed out the difficulties I had encountered in using these colors. I am indebted to Dr. Persifor Frazer for calling attention to my oversight in using them. My apology is, that I selected the colors from the specimen sheet printed in Berlin, and sent out with the American committee's report of the work of the congress. This sheet is entitled the "*Gamme des couleurs (provisoire) pour la carte géologique internationale de l'Europe.*" Upon it the colors for the Devonian are for its three subdivisions, while no colors or modifications of colors are given for subdivisions of the subcarboniferous, and no reference is made to explanations elsewhere. On its face this sheet claims to be complete in itself.

Had I referred, as I see that I should have done, now that Dr. Frazer calls my attention to the matter, to the report of the international committee, and then again to the proceedings of the congress, to ascertain whether or not certain recommendations of the committee were adopted, I should have found that my difficulties had been anticipated, and should have saved myself the trouble of mentioning them. It seems to me, however, that the very fact that such a process is necessary — that one cannot safely use this color-scheme without explanations other than those to be found upon the sheet — is evidence that this system is not all that one might reasonably expect.

As to the purpose of the scheme, I supposed from the first that it was intended for geology the world over; but, after my maps were partly printed, a member of the American committee, to whom I mentioned my difficulty, suggested that these colors were intended only for European geology, and called my attention to the title of the specimen sheet given above.

Dr. Frazer seems to think it unreasonable to expect any system of colors to give entire satisfaction on so small a map. I have no fault to find with the international system on this score, especially as the geology of Indiana is very simple.

The difficulty in subdividing the carboniferous does not come from the scale of the map, but simply from the absence of any fixed method of indicating the subdivisions. To be sure, geologists are left to differentiate as they choose, provided they all use gray; but I may use one method, and another person may use a very different one, the result of which is the absence of uniformity; and uniformity, I take it, is the prime object of a color scheme. In such cases the subdivisions require explanations. My idea of a universal color-system is, that, once introduced, it would need no explanations.

The report of the committee upon the map of Europe suggests that in such a case as the one I refer to in the letter sent out, when the terrane is of a known system, but unknown subdivisions, an initial letter be used in connection with the mean shade of color.

If, instead of colors, we are to use letters, I submit whether we can fairly call such a method of representation a *color* scheme.

JOHN C. BRANNER.

Bloomington, Ind., Nov. 10.

Butter and fats.

Science (Sept. 10, p. 223) says: "Dr. Thomas Taylor's microscopic method for detecting the adulterations of butter with foreign fats seems destined to assume as many shapes as Proteus." Were this even so, it should not excite surprise, considering that about sixty different compositions have been secured under United States patents for butter substitutes, from which it will be seen that oleomargarine has itself become a veritable 'Proteus.' *Science* further says: "At first the globose forms obtained by the boiling and subsequent slow cooling of butter, and exhibiting the Saint Andrew's cross under polarized light, were brought prominently forward as distinguishing marks of pure butter." Answer: What I have stated is, that, when pure butter is boiled, cooled, and viewed as described, globose bodies (butter crystals) appear, exhibiting the Saint Andrew's cross, a fact not now disputed; that lard similarly treated yields a crystal, spinous, without cross; that beef-fat gives a branched and foliated crystal, without cross, — all of which Professor Weber admits, summing up the results of his first three experiments in the following words: "Thus far the results and statements of Dr. Taylor are fully corroborated."

If, however, *Science* intends the inference that I have represented that globose bodies with cross, discovered in any butter-like material when boiled, is a proof that said material is butter, I have only to say that no such idea has ever been entertained by me, or published over my signature. If the inference is intended that the discovery of the butter crystal and cross has some relation to my method of distinguishing oleomargarine from butter, nothing could be farther from the truth. My method of distinguishing oleomargarine from butter consists simply in demonstrating that certain forms of fatty crystals not known to pure butter are constantly found in oleomargarine; and in order to accomplish this, I examine the suspected material, as found in the market, unboiled. By this means I can generally detect at once the lard or other foreign fats, if the material is an oleomargarine. It is manifest that the Saint Andrew's cross found in pure butter would not help me to discover crystals of lard in oleomargarine.

But *Science* says, 'at first.' Am I to understand by the words 'at first' that when I, for the first time, announced publicly that I could detect oleomargarine, it was owing to my discovery of the globose crystals of butter showing the Saint Andrew's cross? If such is the meaning intended, nothing could be more erroneous. I did not discover the Saint Andrew's cross until May, 1884, while the record shows that from July, 1879, until May, 1884, I was determining between butter and oleomargarine by the simple method described. Other helps were sometimes employed, such as testing by acids, boiling to get the odor of butter or other fats, etc.; but I have always considered the presence of highly developed fatty crystals in the material conclusive evidence that the substance is oleomargarine.

In a communication to Hitchcock and Wall's *Quarterly microscopical journal* (vol. ii. July, 1879), published in New York, I set forth, among other statements about butter and oleomargarine, that I was able to detect the latter, owing to particles of cellular tissue, microscopic blood-vessels, and stellar crystals of fat found in it. This paper is illustrated with several cuts, exhibiting respectively the stellar crystals and portions of adipose tissue.

In a bulletin of the microscopical division of the department of agriculture, published in 1884, by direction of Commissioner George B. Loring, a paper of mine appears, with six chromo-lithographic illustrations, two of which relate to the detection of oleomargarine, and show the stellated crystals of lard as seen under the microscope. On p. 6, same bulletin, the following appears: "Aware of the fact that all artificial butter was made directly from crystallized fats, I devised a method by which it could be distinguished from true butter. . . . To carry out this plan, I used the low powers of the microscope with Nicols prisms. In this way I found that I had a method of detecting the crystals, whether in perfect starry form or as fragments of these forms, exhibiting all the colors of the rainbow."

In public debate at the late meeting of the American society of microscopists, at Chautauqua, N.Y., I said that all the convictions obtained in the courts of Washington, D.C., on my evidence, had been founded on my detection of lard or beef-fat in the fatty compounds sold as butter. Thus, first and last, my most important test has been the detection of crystals of foreign fats in butter substitutes sold as pure butter.

On p. 224, *Science* observes further: "Prof. H. H. Weber, however, upon testing the method described by Dr. Taylor, found, that, although the so-called butter crystals could be readily prepared from butter, they could be as readily prepared from beef-fat, or mixtures of beef-fat and lard, under like conditions." Answer: According to Professor Weber's own statement (see bulletin 13 of the Ohio experiment station), he did not use *beef-fat*, but a substance known to the trade as 'oleo,' said to be a manufactured product, containing a much smaller proportion of stearine and palmatine than does beef-fat, and made purposely by oleomargarine manufacturers to resemble butter as nearly as possible in its chemical composition. The professor triturated this butter-like substance with salt and water, boiled it, and when it was cooled discovered that it formed into globose bodies showing a cross; and he says that the crystal thus formed cannot be distinguished from that of pure butter. In this the professor is greatly mistaken. When 'oleo' crystals are observed under a half-inch objective, they can at once be distinguished from butter by their highly spinous character. But, I ask, what bearing has this experiment upon the question of my method of detecting oleomargarine? since crystals resembling those of boiled butter are never found in oleomargarine or butterine as sold.

Science further says (second paragraph): "After the publication of these results, the 'butter crystal' and its Saint Andrew's cross were relegated to a subordinate position." Answer: The Saint Andrew's cross of butter has never been and cannot be 'relegated' from its original position, viz., that of a constant factor of the globose butter crystal; nor can it be used as a means of detecting crystals of lard or of beef-fat in oleomargarine. Pure *unboiled* butter never exhibits either globose or stellar crystals, while oleomargarine and butterine, as sold, show the crystals of fats foreign to butter. *Science* says further: "Dr. Taylor insisted that his most important test has been neglected, viz., the appearance of the unboiled material under polarized light with selenite plate. According to Dr. Taylor, butter shows a uniform tint, while lard and tallow show prismatic colors." Answer: The assertion that the above is my most important test is found nowhere in my writings. In my open letter to Professor Sturtevant of the New York experiment station (March 21, 1886), I say: "The crystals of lard or of tallow generally observed in great numbers are easily distinguished from the mass of amorphous fats with which they are combined. This is one of my most important tests of oleomargarine and butterine." My assertion, 'This is one of my most important tests,' is thus made the foundation of a statement that something else is my most important test. In my publications relating to the detection of oleomargarine, from 1879 to the present time, I have reiterated the necessity of finding in the suspected material crystals of foreign fats in order to prove beyond a doubt its spurious character. *Science* further says: "Here again, however, he [Dr. Taylor] has been pursued by Professor Weber, who shows that either butter-fat or lard or tallow, when cooled quickly, will show a uniform tint, while if cooled slowly, so as to admit of the formation of larger crystals, prismatic tints are shown by both. Since imitation butter is . . . liable to undergo sufficient changes of temperature after manufacture to allow of a partial re-crystallization, the test is plainly fallacious." As regards the first sentence of the above quotation, it may be stated that *large crystals* of *butter* can never be found in unboiled oleomargarine, from the very nature of its manufacture, since the only butter it contains is derived from the milk with which it is churned. In the manufacture of butterine, however, butter, melted at the lowest possible temperature, is added to liquid 'oleo' and 'neutral lard' and churned. Even in this case the butter does not crystallize. Were the butter melted at a high temperature, its odor and taste would be objectionable; it would also crystallize in large globose forms, giving the butterine the granular appearance of lard, which would render it unsalable.

In the latter sentence of the above quotation, *Science* acknowledges that imitation butter is liable to undergo sufficient changes of temperature after manufacture to allow of a partial re-crystallization. For years past I have been endeavoring to convince

those interested in this subject of this very fact thus acknowledged by *Science*. But be it remembered, that, in the re-crystallization that takes place after manufacture, it is not the 'oleo' crystal with cross that re-appears, but a stellated body resembling lard. Normal butter always shows a uniform tint; lard and tallow, as sold everywhere, show prismatic colors. What Professor Weber alludes to is strictly neither lard nor tallow, but a specially prepared material known as 'oleo' and 'neutral lard.' These he chills suddenly to prevent crystallization, a condition not suggested by the broad statement contained in my paper. No unbiassed mind would compare the evanescent results of this experiment with an ounce of 'neutral lard' or 'oleo,' with the constant crystalline condition of the million of pounds sold daily in our markets.

With regard to the optical test of oleomargarine observed in the use of polarized light and selenite plate, I have said: "If the sample is submitted to the action of polarized light and selenite plate, and appears of a uniform color according to the color of the selenite used, we have another *indication* that the substance is *pure normal butter*, which, under these conditions, never exhibits prismatic colors. Sometimes large crystals of salt cause the appearance of prismatic colors in pure butter, by refraction: these should be removed. Butter that has been exposed to light until it is bleached, or butter that has been in immediate contact, for a long time, with a substance that absorbs its oil, as when placed in wooden tubs, has undergone a chemical change, and should not be considered as normal butter" (extract from the Sturtevant open letter, which Professor Weber professes to have reviewed). But even butter of this description never exhibits crystals resembling those of either lard or 'oleo.' The prismatic colors of an abnormal butter, described by Professor Weber, and accounted for by me in my earlier papers as observed in decomposing or over-heated butters, etc., could not be mistaken by any but a novice for the gorgeous tints seen, with and without the aid of selenite plate, in butter substitutes in general. In a letter addressed to me, April 8, current year, Professor Sturtevant says: "Your claim for the selenite plate received our attention a long time ago, as we observed it in Professor Wiley's report for 1884. This test seems to offer promise of value." Professor Wiley, chemist of the department of agriculture, says: "Pure unmelted butter, when viewed through a selenite plate by polarized light, presents a uniform tint over the whole field of vision. On the other hand, butter substitutes give a field of vision mottled in appearance. This phenomenon is so marked, that, with a little experience, the observer will be able to tell a genuine from an artificial butter with a fair degree of accuracy. While the examination should never stop with this optical test above, it can be advantageously used as a preliminary step." My bulletin was issued in 1884; the agricultural report for 1884 was issued in 1885.

In a footnote to my paper already mentioned (Hitchcock and Wall's *Journal*), the following appears: "Well-made oleomargarine may be quite free from any crystalline appearance, at least while fresh. . . . The sudden cooling on ice seems to prevent the immediate formation of crystals, but it is not unlikely that these will gradually form in course of time." Thus it is shown that Professor Weber was anticipated by seven years in this case. A tub of fresh oleomargarine, direct from Armour's factory, Chicago, the present month, was examined as soon as received. Stellated crystals were at once observed in it, and the entire field was covered with prismatic colors.

Professor Weber states that a sample of butter subjected to heat and cold in his laboratory, but which did not actually melt, showed under the microscope prismatic colors, and he pointedly, although mistakenly, asserts that this butter fairly represents the condition of butter generally. In a paper read before the American society of microscopists, August, 1885, published in the Proceedings of the society, I say: "When oleomargarine or butterine is newly made, crystals of fat are seldom observed in it when viewed under the microscope; but in course of time, owing to its being subjected to light and increase of temperature in stores, it exhibits crystals of fat more or less. In butter substitutes of commerce the crystals are seldom absent."

Science further says: "Apparently, Dr. Taylor prepared his annual report with these results in mind, for there, and in his paper before the annual meeting of the American society of microscopists at Chautauqua, Aug. 10–16, he gives his method a still different exposition." Answer: The most important part of this sentence, to me, is its personal character. It contains an indirect charge that I so altered my official report to the commissioner of agriculture as that it might appear that I had anticipated Professor Weber in his novel views and experiments. It is sufficient to say that my official report was placed in the hands of Colonel Nesbit, chief clerk of the department of agriculture, at least six months before Professor Weber made his experiments. The points to which *Science* alludes are all contained in my report to Professor Kellicott, secretary to the American society of microscopists, at Buffalo, N.Y., sent him by mail Oct. 7, 1885, and were not afterwards altered by me, as the publishing committee will testify. Independently of all this, there is on file in the department of agriculture a copy of my original report, made by one of the clerks of the statistical bureau, over one year ago, which agrees with my published official report. *Science* further says: "Dr. Taylor's first step is now to search for fat crystals in the test sample by plain transmitted light." Answer: As has been shown, this was my method for the first several years, for the simple reason that lard crystals are by this means easily detected, but I subsequently discovered that the crystals of beef-fat could not be properly defined without the aid of polarized light. *Science* further says: "By the application of polarized light, 'amorphous crystals,' whatever these may be, may be detected." Answer: I have applied this term, 'amorphous crystals,' to mottled fats which, seen by polarized light without selenite, exhibit no particular form or structure, but, seen by polarized light with selenite plate, exhibit specks and prismatic colors, thereby showing their crystalline condition. *Science* further says: "To determine whether these amorphous crystals are of beef-fat or of lard, the sample is boiled and slowly cooled, as already described, and mounted in oil." Answer: In my official report I say: "Having *first* examined the suspected material under the microscope, it may be boiled." The precaution of a preliminary examination by polarized light is highly necessary, for, should the sample contain a large per cent of butter, boiling might cause it to crystallize in large globose bodies,

by which the small crystals of lard and other fats might be absorbed and thereby escape detection. In the case of a true oleomargarine, which consists almost wholly of 'oleo,' the process of boiling would develop beef-fat crystals without cross, which would not be modified in form by the small quantity of butter in the compound.

Science further says: "Under these conditions, he now finds, in accordance with Professor Weber, that butter, lard, and beef-fat all give globular crystalline bodies which (apparently with the exception of lard) show the St. Andrew's cross." Answer: *Science* is misinformed in this case. The above statement is not in accordance with the facts. Professor Weber's language, in bulletin 13, is: "The butter revealed a well marked black cross;" "the lard, small irregular stellated bodies;" "beef-fat, only small stellate crystals." The last is an erroneous description of beef-fat, however, which has a branched and foliated crystal. It must be confessed that Professor Weber has an odd way of 'corroborating' the correctness of my experiments, — employing 'oleo oil' instead of rendered beef kidney fat, according to the statement in my 'abstract.' 'Oleo,' a substance not mentioned in my experiments, is no more beef-fat than phenic alcohol is coal-tar, although the one is a product of the other. *Science* says: "The above account of Dr. Taylor's method, as at present described by him, is drawn mainly from his last annual report to the commissioner of agriculture." Answer: *Science* is in error on this point. The points referred to by *Science* are taken mostly from my open letter to Professor Sturtevant, and from Professor Weber's bulletins 13 and 15, of the Ohio experiment station. My method of detecting oleomargarine has nowhere appeared in the columns of *Science*, nor in the reports of Professor Weber. My official report for 1885 was not issued when Professor Weber published the paper of March 1, 1886, nor does he seem to have been aware of my other publications mentioned in this paper. In point of fact, Professor Weber, unfortunately, undertook to discuss my method of detecting oleomargarine, by reviewing an abstract that did not so much as mention the subject. In conclusion, *Science* says: "We shall endeavor to keep our readers informed of the changes which the method undergoes in the future." This last is to me the most gratifying sentence in the whole article.

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Anemometer exposure.

I have been allowed space in recent issues of *Science* to call attention to errors which may arise from the position of thermometers and barometers relative to surrounding objects: may I now call attention to similar errors which may arise from badly placed anemometers? The subject is not a new one, but I wish to urge the necessity of a more uniform exposure than that now used by our signal service. According to the Associated press reports of the storm of Oct. 14 and 15 in the lake region, the wind tore through the trees of the Chicago public parks, on the morning of the 14th, with the fury of a hurricane, twisting saplings off and hurling them over the tops of large trees, littering the streets with broken trees and shattered sign-boards, and demolishing at least two buildings; and all this, according to the same despatch, while the wind was "blowing with a velocity of 20 miles an hour." Similar reports came from surrounding towns. The production of all this damage by a 20-mile wind seemed so absurd that I wrote to the signal officer at Chicago for the observed wind velocities on Oct. 14, and received the following: "Oct. 14, 1886, max. vel. of wind, S.W., 27 at 12.58 P.M.; vel. at 7 A.M., S.E., 11; at 3 P.M., S.W., 28; at 11 P.M., S.W., 11." Wind velocities of 40 miles per hour are not unfrequently recorded in Boston. On Oct. 31 the anemograph at the Boston signal office showed a maximum velocity of 40 miles, and on April 6 a maximum velocity of 51 miles; yet in neither case was there any record of broken or overturned trees and injured or wrecked buildings. This seems to show that wind velocities reported from Boston cannot be compared with wind velocities reported from Chicago. Not only can we not compare two stations of the signal office together, but we cannot compare wind velocities obtained during different years at the same station. During recent years high wind velocities have been much more frequently recorded at the Boston signal office than previously, and we find that while the average monthly wind movement at Boston from 1870 to 1881 was 6,630 miles (see Report chief signal office. 1883), the mean monthly movement during the last two years has been 8,120. Are we hence to conclude that Boston is becoming a windier place? I think not. The signal office at Boston was moved from one building to another building in 1884, and since then the velocities have been higher than previously, and are no doubt due to the changed position of the anemometer. But even with a continuous exposure of an anemometer at the same place, it is doubtful, as anemometers are now exposed, whether wind velocities from different directions can be compared with one another. There are two anemometers — a Draper and a Hahl — on the tower of the observatory at Blue Hill. These rise about eleven feet above the roof of the tower and about eight feet above the parapet. The Hahl anemometer is situated on the south side of the tower, and the Draper on the east side of the tower, which is sixteen feet in diameter. During the last three months there have been seventeen days on which the prevailing wind was from the west; and on all of these except four the total daily movement shown by the Hahl was larger than that shown by the Draper. On these seventeen days the average daily movement shown by the Hahl was 438 miles, and by the Draper 426. During the last six months there has been ten days on which the prevailing wind was from the north, and on all but three the Draper recorded more than the Hahl. On these ten days the average daily movement shown by the Draper was 353 miles, and by the Hahl, 346. This seems to show that wind velocities from different directions recorded by either instrument cannot be compared with each other, though the differences in this case are not large. Yet I think the Blue Hill anemometers are better exposed than many of those of the signal service which are near the edge of tall buildings, and have an abrupt descent on one side of them, and a long roof or series of roofs on the other.

As a sequel to this, I might call attention to the large errors which may arise from the bad exposure of the signal service rain-gauges on roofs, but I think this is generally recognized.

H. HELM CLAYTON.

Blue Hill meteor. observ., Nov. 10.